the high temperature of the continents exert a considerable influence. Then occasionally a continuous return of polar air may be established along the west coast of the continents and leading direct into the trade winds.

These results can not fail to exert a considerable influence upon the methods of weather forecasting. All meteorological events of the Temperate Zone, great and small, derive from the described great atmospheric circulation, as we know it from the motions of the polar front. If we succeed in watching it effectively, it should be possible to give the short-range forecasts a hitherto unattained accuracy. And it should be possible to complete them by long-range forecasts giving the general character of the weather perhaps for weeks ahead. And these two kinds of forecasts could be extended to all regions of the Temperate Zone, to oceans as well as to continents. The required survey of the polar front is merely a question of organization.

551.524 (048)

PROPAGATION OF COLD AIR ON THE SURFACE OF THE EARTH.[1]

By F. M. EXNER.

[Reprinted from *Science Abstracts*, Sect. A, Sept. 30, 1920, §1137.]

The general problem for solution is: Given a mass of dense air of known form and state of motion, covering a small region of the earth's surface, and bounded above and around by less dense air also in known state of motion, to determine the subsequent movement and changes of form of the dense air. The problem is solved for some particular cases in two dimensions, by the hydrodynamical methods applicable to waves at the surface between two fluids of unequal density, but the results sought are mainly qualitative. The dense air is taken initially in the form of a ridge of rectangular cross-section and infinite length. If both fluids are originally at rest, and friction and the earth's rotation are neglected, then the ridge breaks up into two of equal breadth and half the height of the original, traveling in opposite directions perpendicular to their length with a velocity proportional to the square root of the difference of absolute temperature between the cold and warm air. As the ridges move apart the warm air flows in and covers the region of the earth's surface between them. Introducing friction, the ridges separate as before, but decrease in height as they advance, the space between them remaining covered with cold air. In this case, too, the rear of each advancing ridge is higher than the front. On a rotating earth without friction the cold ridge, supposed streaming in the direction of its length, breaks up as before, but the ridge traveling to the left of the direction of streaming (Northern Hemisphere) increases in height, while the other decreases. The velocity of propagation is now greater than before, and the front of each ridge higher than the rear. If the warm air above is streaming at right angles to the ridges, it has the effect of checking the advance of one of them, and may, if strong enough, reverse its motion and make it slowly follow the other, which has its velocity of propagation increased. Other cases are obtained as combinations of these. A comparison is made with observations on the spreading of cold waves over Europe and North America. The author finds in this work an explanation of the phenomena exhibited on these occasions, and in particular of the observed fact that when cold air breaks through from the polar regions it first seeks to spread W. or SW., then S., SE., E., and often finally NE.—*M. A. G.*

[1] Akad. Wiss., Vienna, vol. 127, 2a, 1918, pp. 795-847.

551.515 (048)

THE ENERGY OF CYCLONES.

In several issues of *Nature* (London) late in 1920 there has appeared a running discussion of the source of the energy of cyclones. There is presented such a concise nontechnical summary of the present ideas of British meteorologists on this intricate subject that extensive quotations are reprinted here. The discussion was started by adverse criticism of Margules's theory by R. M. Deeley. In an obituary of Dr. Max Margules, published in *Nature* (London) October 28, 1920, pp. 286–287,[1] E. Gold gives the following short summary of Dr. Margules's discussion of the energy of storms:

> Margules contributed to the Year Book of the Meterological Institute of Vienna for 1903 a comprehensive discussion of the energy of storms. He showed that the atmospheric phenomena associated with storms would arise if two masses of air of different temperatures were in juxtaposition. The situation would be unstable, and in passing from this unstable situation to a stable one the potential energy would be reduced, part of it being converted into the kinetic energy of the ensuing "storm." This paper contains the germ of the theory of squalls, of the development of cyclones, of polar fronts, and so forth. It concludes computations of the horizontal velocities which would result from various distributions of pressure and temperature, and shows that actual distrib tions would lead to velocities of 50 miles an hour. Margules summed up his conclusions in the sentence: "So far as I can see, the source of storms is to be sought only in the potential energy of position."

Mr. Deeley, in a letter to *Nature* (Nov. 11 issue, p. 345), criticises many of Margules' points, and concludes with the following sentence:

> The facts seem to point to the stratosphere as being the main source of energy of storms and trade winds.

To this Lieut. Col. Gold replies (ibid.):

> Dr. Margules wrote his paper mainly in connection with phenomena of the line-squall type, but he realized that it might have wider applications, and later investigations do indicate that discontinuity of temperature is the prime factor in the "birth" of cyclones. If one had an atmosphere with uniform pressure at sea level, but with masses of warm and cold air, then at 9 kilometer pressure would necessarily be low in the mass of cold air, and a cyclonic circulation would ensue; but the enrgy of motion would be derived from the potential energy of the initial state.
>
> Differences of temperature originate in the lower atmosphere. The stratosphere may be able to draw upon a source of energy of which we are ignorant; it can not of itself provide the energy of storms.

In the next issue of *Nature* (Nov. 18, pp. 375–376), W. H. Dines presents the following further discussion:

> It does not seem to me as though any really satisfactory theory has yet been put forward to explain the genesis and maintenance of cyclones; I fully agree with Mr. Deeley (Nov. 11, p. 345) that they are not due to contiguous masses of air at different temperatures but, on the other hand, I do not see how they can originate in an inert and stable region like the stratosphere.
>
> Were storms produced by contrasts of temperature—or in other words, by the so-called polar front—surely they would be most violent where the contrast was most marked. The stormiest parts of the world are the great belt of the southern ocean from 40° to 60° S. latitude, and that part of the Atlantic which lies northwest of Scotland, and neither of these regions shows any exceptionally steep gradient of temperature.
>
> Observations in the upper air have shown a remarkable uniformity in the mean temperature (mean with regard to height) from 0 to 20 kilometers in every place where they have been obtained, and it follows as a corollary that there is a very uniform pressure at 20 kilometers height over the globe, for the pressure at 20 kilometers is almost independent of the surface pressure. Observations over Europe, the only part of the world where they are numerous enough for the purpose, have also shown a most extraordinarily close correlation between the temperature and pressure of the air in the upper part of the troposphere,

[1] Abstract published in *Mo. Weather Rev.*, Oct., 1920, 48:601.

many of the coefficients exceeding 0.90. These facts must be reckoned with in any theory about the formation of cyclones.

My own belief is that pressure differences originate in the upper half of the troposphere from variations in the strength of surrounding winds. Being given the means of originating and maintaining a difference of pressure at about the height of 9 kilometers the rest of the phenomena would follow readily. The distribution of temperature, the high positive correlation below and the negative correlation above, and the rise and fall of the tropopause between cyclone and anticyclone are all explained by the vertical motion of the air that would naturally follow from the distribution of pressure.

A short note by Sir Oliver Lodge followed (Nov. 25, p. 407):

I do not find that people in general are aware of an important source of energy for the maintenance and intensification of cyclones, nor am I acquainted with a clear exposition by a meteorologist that the condensation of aqueous vapor will suffice.

Atmospheric pressure being a ton per square foot, the disappearance or collapse of a cubic foot of ordinary air would yield a foot-ton of work. The disappearance, by complete condensation, of the aqueous vapor in 760/12.7, say 60, cubic feet of atmosphere would yield about the same amount.

If, then, the temperature of saturated air fell from 18° to 12° C. by reason of condensation and rainfall, so that the vapor pressure diminished from 15.36 to 10.46 millimeter of mercury, a foot-ton would be generated in each 155 cubic feet of that region of the atmosphere. Incidentally, the corresponding deposit of liquid would be 5 grams per cubic meter, or a rainfall of one-third inch from a vertical mile of air.

Assuming that the above fall of temperature in the central region of a traveling cyclone is not excessive, the energy available in each cubic mile of it would be nearly a thousand million foot-tons.

Immediately following this is a note by J. R. Cotter suggesting "that the energy of a cyclone is derived from the heat energy of the earth's surface." Sir Napier Shaw, Harold Jeffreys, and L. C. W. Bonacina contributed further to the discussion in the issue of December 2 (pp. 436–438):

There can be no doubt, I suppose, that solar and terrestrial radiation are ultimately responsible for the kinetic energy of the winds. The doubts expressed by Mr. R. M. Deeley in *Nature* of November 11 and by Mr. W. H. Dines in the issue of November 18 can refer only to the details of the phenomena consequent on the process of transformation of the energy. The first stage is obviously the storage of energy in the potential form of air charged with heat and moisture at the surface or lower levels and cooled by radiation at high levels, especially in the polar regions, as on the plateau of Greenland or on that of the Antarctic continent, or on the sunless slopes of the Himalaya. Equally without doubt the next step is convection, the greater part of which is indicated here and there by falling rain or snow. Measurements of rainfall assure us that there is no lack of energy available for violent winds if the heat engine is at all efficient.

The general effect of the process of convection is the development of a vast circulation in the upper regions of the atmosphere from west to east round the poles, which has its counterpart in the normal distribution of pressure at corresponding levels. That is probably most pronounced at a level of 8 kilometers because at that level density is equal all over the globe at all seasons of the year. Above that level, up to the level of equal pressure at 20 kilometers of which Mr. Dines writes, there is, on the average, a gradient of density from the Equator to the pole, and below the level of 8 kilometers a gradient of density in the opposite direction. The layer of maximum average velocity is above the layer of maximum pressure gradient on account of the diminution of density with height.

Below the level of 8 kilometers the distribution of pressure is affected by the gradient of density in a very irregular manner, because the distribution of land and water is irregular. The net result at the surface is the complicated distribution of average pressure which we find in the maps of normals for sea-level.

The maintenance of the average general circulation from west to east in the higher levels is due to the gradual convergence toward the polar areas from which the cooled air flows. That must obviously be balanced by a corresponding flow toward the Equator, and as poleward flowing entails a westerly circulation, so flowing toward the Equator entails an easterly one. We must, therefore, find room in the system for a body of air flowing from the east comparable at least with the circulation from the west. We find such a body of air in the great easterly circulation of the intertropical regions, which is naturally stowed away over the Equator as far as possible from the centers of the two polar demi-hemispheres of influence of pressure gradient.

These great circulations, easterly and westerly, form a normal "groundwork" of all atmospheric motion; and when Mr. Deeley and Mr. Dines write of the energy of cyclones, they are not concerned, I think, with the energy of the general circulation of the upper levels which I have described, but with the minor circulations which represent the perturbations of the major circulation.

I think myself that the convection of warm, moist air, combined with the vagaries of temperature in the lower layers, will, in the end, prove to be sufficient to explain the energy of cyclonic air currents—whether directly or as the secondary effect of current-differences, I can not say. Probably, in order to get a correct view of the perturbations, we ought to subtract vectorially from the observed winds the local motion of the normal circulation, or else accustom ourselves more than we do to the theoretical combination of local circulation with a general circulation.

There are four other aspects of the problem upon which we are at present almost uninformed. The first is the locality where the cyclone, which is the subject of study, was generated; just as the cyclone itself is a perturbation of the general circulation, so what we see going on over our heads is the perturbation of a cyclone which may have originated in the general circulation thousands of miles away. A cyclone is a more or less stable dynamical system which certainly travels, but changes as it travels. The second aspect is the variation of velocity of the wind with height in the general circulation and in the cyclonic area itself. The third, which is closely connected with the second, is the trajectory of convected air. This could be calculated if we knew the point from which it started and the variation with height of the current which carried it. One often reads of convected air rising vertically, but we know that the actual trajectories of a pilot balloon are of very various shapes, seldom vertical, and the balloon may part company from the air which supported it at the start by a distance measured in tens of kilometers. Air in convection rises very slowly. If we set its vertical velocity at one-hundredth of that of a pilot balloon, the convected air may be thousands of kilometers from the starting point before its upward journey is finished, and its path may be very complicated. It is possible that this conception of the slow, gradual ascent of air may have a bearing upon the cloud formation associated with a coming cyclone, but the subject is too long for a letter.

The fourth aspect is the behavior of the convected air with regard to its environment. The slowness of its rate of ascent is dependent largely upon the development of eddies and consequent dilution of its mass with the cooler environments. This can not of itself arrest the upward motion, though it delays it, and, consequently, when the convected air has arrived at its ultimate level it will have carried with it some of the air which formed its environment on the way. Hence the rising air will have "evicted" a certain amount of air by its passage.

The importance of combining these aspects is at once apparent if we consider that convection in still air would simply mean a readjustment of the mass in the vertical. The potentially warm air would be at the top instead of at the bottom, and the effect of a completed process of convection would be that pressure would rise within the area of operations. But if the risen air were delivered into a rapidly moving current at the top, the air which it had "evicted" from the environment on its way would be lost to the column, and when the process was completed the air would close in from the top, the bottom, and the sides. If there were any relative motion to begin with—and there is always some—closing in from the sides must develop cyclonic circulation with a cold core. Closing in from the bottom with air colder and drier than that which began the convection would stop on account of dynamical cooling, and closing in from the top means the settling down of the air of the stratosphere and a consequent low tropopause with a column of air above it warmer than its environment.

These conditions describe what the late Lord Rayleigh postulated for superposing a vortex on a current with relative velocity of its parts. They also agree with what Mr. Dines describes as the results of his examination of actual cyclonic conditions in England. And this view of the procedure is borne out by the examination of tropical cyclones. We can form legitimate inferences from the pressure records of these visitations because the normal conditions of the localities where they occur are extremely regular. We can see by an inspection of the graph of pressure that the region covered by a cyclone has simply lost a certain part of the air which it normally possesses. In one example I estimated the loss as equivalent to 40,000 cubic kilometers at sea level. Beyond all doubt or question air had gone; it was not piled up in anticyclones fore and aft, as we used to think the convected air of our cyclones must be; it was gone clean away. I suspect that it travelled away in some upper current until slowed down over the tropical anticyclone of some ocean. The story will not be complete until that surmise is verified or the correct account substituted. Hence, for the time being, I am as curious about the life history of convective air currents as I was twenty years ago about that of surface air currents.

In any case, it seems to me certain that, because it carries away part of the air which it meets on its path, convection, wherever it occurs, must entail convergence, and therefore, except at the equator, it must give rise to a cyclonic circulation which may be transient, or, if circumstances are favorable, permanent. The function of the stratosphere seems to be not constructive, but conservative and registrative. It protects the energy from being dissipated by "filling up," because the descent of its isothermal air is arrested by the adiabatic rise of temperature. That is, indeed, the common function of all "decks" or lids in the atmosphere, of which the stratosphere is the chief. At the same time, for an observer the stratosphere registers the locality of low pressure by the lowness of the tropopause and the relative warmth of the air column above it. It seems to be a law for the general circulation and for local circulations that as pressure diminishes in the troposphere the tropopause is lowered and the temperature of the columns above it rises.

Consequently, my view at the present time is that the energy of a cyclone is due originally to convection in a region with a suitable law of variation of velocity with height; it is guarded at the top by the isothermal condition of the stratosphere, and on the sides by the balance of pressure and rotation. It is open to slow attack at the bottom on account of the friction of its winds with the surface, and unless its energy can be maintained by additional convection it must perish. I do not think that a traveling cyclone carries its supply of rain for long distances; it probably manufactures it out of the material in the lowest levels which it has to pass over. But it uses the energy so supplied first to form a secondary, and afterwards to absorb it or to be absorbed by it.—*Napier Shaw.*

It is a well-known hydrodynamical result that, in the absence of any external stabilizing influence, any surface of discontinuity of velocity in a fluid must be unstable. The effect of this instability is seen in the eddies produced in a mill pond, at the margin of the entering stream. A sufficiently rapid shearing, without actual discontinuity, will produce the same effect. Most atmospheric eddies are developed in this way. In the case of differences of velocity between different masses of air at the same level, gravity is not directly available to damp any eddies that may be produced, and hence it does not seem likely to be difficult to account for eddies with their axes vertical.

Thus the origin of cyclones may well be explained on the lines suggested in Mr. W. H. Dines's letter in *Nature* of November 18. It is rather more difficult to see what determines the size and intensity to which they grow. Ground friction must play its part; also, where the warm stream on the south side bulges northward, it must do so to some extent over the top of the cold air already there, and this arrangement makes for stability, and when sufficiently developed must prevent the further growth of the disturbance.

The speed of translation of the cyclone on this theory should be the mean of the velocities of the two currents, which is usually about correct. The geostrophic condition must also hold approximately, otherwise the disturbance would spread out with nearly the velocity of sound and disappear. What is not easy to see, however, is why the isobars tend to become more or less circular instead of wavy.—*Harold Jeffreys.*

I should like to express my agreement with Mr. W. H. Dines's view (*Nature*, Nov. 18, p. 375) regarding the origin of the initial difference of pressure which leads to the development, under the influence of the earth's rotation, of cyclonic circulation, and to state that I have often suggested that this initial disturbance may have a mechanical origin (see Quart. Journ. Roy. Meteor. Soc., vol. xliii, 1917, p. 27). At the same time it seems that one can not, on many grounds, ignore the effect of temperature contracts as a contributing factor in the further development and maintenance of storm energy.

To take the very fact which Mr. Dines cites, namely, the exceptional storminess of the Atlantic to the northwest of Scotland. This region is, in a most conspicuous degree, stormier in the winter months than in the summer, and it is almost one of the canons of physical geography that the excessive development of storm energy during the cold season is favored by the great contrast in temperature between the frost-bound continents and the warm Atlantic, the individual cyclonic systems breeding not so actively over the land areas, where the general pressure is high, as over the oceanic areas, where the general pressure is low. On the other hand, during the warm season—when the temperature gradient between the oceans and the continents is reversed, but is much less steep than the winter gradient—cyclonic energy in the North Atlantic is far less powerful, while over the sun-heated continents storm energy takes the form, not of extensive wind systems, but of localized convectional thunder systems. Furthermore, in the southern ocean, between 40° and 60° S., where there are no disturbing land masses, there does not appear, judging from the reports of navigators, to be such conspicuous seasonal difference in storminess, and this is borne out by statistics available for the Falkland Islands (Meteor. Office Geophys. Mem., No. 15).—*L. C. W. Bonacina.*

A rebuttal by Mr. Deeley is published in *Nature*, December 16, 1920, page 502, and adverse comment on it by W. H. Dines in the issue for December 23, page 534. The second paragraph of Mr. Dines's letter relates to the earlier comments of Sir Oliver Lodge:

> The point mentioned by Sir Oliver Lodge in his letter in *Nature* of November 25, has been, I think, put forward by von Bezold and others, but Sir Oliver seems to have overlooked the result of the heat set free by the condensation of the vapor. Could a cubic meter of damp air be confined in an adiabatic but extensible balloon and the vapor be condensed by any means, the result would be an increase of volume, for the expansion due to the heat produced by the condensation would far more than balance the contraction due to loss of pressure. If, indeed, the heat energy due to the latent heat of vapor all took the form of kinetic energy in the atmosphere, quite a trifling rainfall would suffice to produce over the same area the most violent cyclone ever recorded.

Prof. Alexander McAdie having become particularly interested in Sir Napier Shaw's discussion, wrote to him and obtained further comments. At the close of a discussion [2] of Shaw's published letter (quoted above), Prof. McAdie says:

> Finally, one further proposition appears to Shaw to be worth study, namely, that "an anticyclone is a region of descending air if the month is the unit of time; but if the unit of time is the hour or day, an anticyclone is simply an unchanging mass except for the outflow at the bottom." This outflow he has calculated (in a letter to the writer) as a settlement at the rate of 100 meters per day, about 0.7 m/s. In a cyclone per contra, the air rises at such a rate that the hour and the day are units of time.
>
> This conception of the time factor is novel, and if we permit its introduction, as it seems we must, then we are faced with the further problem of determining the life histories of slow-moving anticyclonic air descending from heights of 8 or 10 kilometers, most leisurely, requiring 6, 8, or 10 days to make the downward passage and landing far, far away from the place of setting out. In the case of hyperbars [large subpermanent high-pressure areas], such as the Atlantic [or Azores] anticyclone, Shaw says: "The out curvature is everywhere *the same angle;* and the velocity is proportional to the distance from the center. If $V=Cr$ and the velocity normal to the circle is proportional to r, the outward velocity will be $C'r$ and hence the outflow $\alpha 2\pi r^2$ is proportional to the area; and therefore implies a uniform settlement all over the area."

Much more will be said about cyclones and anticlones and the general circulation of the atmosphere when we have free-air data from more than a few small sections of the vast regions traversed by moving low and high pressure areas.—*C. F. B.*

551.524 (77)

THE RAPID FALL IN TEMPERATURE OF COLD WAVES.

A high pressure area will sometimes form over night, especially in or over the Lake region, where the temperature the day before was about normal for the season all over the district. Immediately the temperature falls with the accompanying northerly winds perhaps 20° or even 40° [F.] in 12 or more hours. This is quite a common occurrence in this section [northern New York] in the winter. This air could not have had time to lose so much heat by *radiation*, neither could the wind at the surface have had time to *import* so low a temperature. There are many clear nights with high barometer, with every opportunity for radiation to cause a big drop in temperature, but no such fall is observed other than perhaps 10° F. Last night [Jan. 26–27, 1921] under a clear sky and high pressure the temperature actually rose several degrees and without wind movement. A perfect calm prevailed. Where did this heat come from? For two nights before this, while under the influence of the northerly wind area, and only a fresh to light wind at that, the temperature rose but little during the day and fell during the night more than it would under ordinary conditions. * * * This coldness of the air does not feel as if it were due to long brooding or stagnation over cold

[2] Presented (by title) before the American Meteorological Society at Chicago, Ill., Dec. 29, 1920. Abstract published in *Bull. Am. Met. Soc.*, March, 1921, 2: 40.